

have also, after a still more complete examination, come to the same conclusion; so this species must stand in the 'Birds of India' as *TURDUS VISCIVORUS*.

It is interesting to note that the Kashmir name of this Thrush is *Eili-kau* or *Eili-chettar*, meaning Mistle-crow and Mistle-eater. One I killed in Gulmurg measured 11 inches in length, $18\frac{1}{2}$ in extent of wing. It breeds abundantly both in Kashmir and in the Sutlej valley.

370. *OREOCINCLA MOLLISSIMA*.

This Thrush also extends to Kashmir, and has, indeed, been sent from Moupin by Père David.

The feathers of the rump in this species are distinctly spinous.

371. *OREOCINCLA DAUMA*.

One killed at Darjeeling measured in the flesh $10\frac{3}{4}$ inches in length, extent $17\frac{3}{8}$, wing $5\frac{3}{4}$, tail nearly 4, tarsus $1\frac{1}{4}$, foot 2.

A very closely allied bird from Formosa, stated to be larger and somewhat paler, was named *O. hancii* by Swinhoe, but is now considered by him identical.

372. *OREOCINCLA NEELGHERRIENSIS*.

The *Zoothera imbricata* of Layard, from Ceylon, turns out to be the same as this bird.

[To be continued.]

XVI.—*On the Flight of Birds.*

By Captain F. W. HUTTON, C. M. Z. S.

THE mechanism of flight has lately been very fully and ably discussed both in England and on the continent of Europe; but considerable obscurity seems still to exist as to the actual movement of the wings of birds when flying.

Mr. Macgillivray ('British Birds.' vol. i. p. 43) says that the effective stroke of the wing is delivered downward and backward, and that the resistance of the air bends upward the tips of the feather and in this way gives a forward impulse to the bird. The opinion of the Duke of Argyll appears to be the same ('Reign of Law,' p. 142 &c.); but he says (more correctly, I think) that the stroke is delivered directly downward. The experiments of Dr. Pettigrew, however, on sparrows with cut

wings, show that progression is not obtained by the uplifting of the free ends of the feathers; for their flight was apparently in no way impaired when the free ends of the feathers were cut off.

Dr. Pettigrew, in his admirable paper on the subject (*Trans. Linn. Soc.* xxvi.), asserts that the effective stroke is downward and forward, and that by a peculiar twisting or screwing motion of the wing (which I am bound to confess I do not quite understand) the air is forced to escape near the root of the pinion, between the secondary and the tertiary feathers, in a downward and backward direction, and that the reaction thus produced supports the bird and drives it forward. Dr. Pettigrew's own experiments, however, hardly support his theory; for both No. 12 and No. 13 (p. 220) show that the feathers forming the funnel by which the air is supposed to escape, are not necessary for flight, while No. 18 proves that, although the secondaries may be complete, flight is prevented by cutting off the ends of the primaries; that is to say, his experiments show that flight in reality depends principally upon the primary feathers, while his theory makes it depend principally upon the secondary ones. Both Dr. Pettigrew and Mr. Macgillivray consider that the wing is extended during the down-stroke, and more or less folded during the up-stroke; and Dr. Pettigrew and the Duke of Argyll agree that the wings during progression describe a "wave-track," or undulating line in the air. But the ingenious experiments of Prof. Marey (*Ibis*, April 1870) appear to show that during the down-stroke the wing moves first slightly forward, then more and more backward—and in the up-stroke, at first backward and then forward into its original place again, thus describing, during progression, a cycloidal curve in the air; also that during the greater part of the down-stroke the wing, by turning on its axis, slopes forward and downward, while during the up-stroke it slopes forward and upward—thus being, on this point, quite opposed to Dr. Pettigrew, who states distinctly (p. 255) that during the down-stroke no depression of the anterior margin and elevation of the posterior one takes place. Under these circumstances a few observations that I have made on the motion of the wings of the Sea-gull during

flight may perhaps prove of interest ; for I think that they will reconcile many of these discrepancies, as well as explain all the anomalies observed by Dr. Pettigrew in his experiments with sparrows, and at the same time will supply a theory of flight much simpler than any of those hitherto proposed. My observations were made on the Black-backed Gull of the southern hemisphere (*Larus dominicanus*) ; but doubtless they will apply to all other Gulls, and probably even to all other birds.

No better opportunity occurs of observing the movements of the wings of a living bird in a free state than when on board a steamer steaming head to wind, and surrounded by a flock of Gulls. The Sea-gull is a bird that moves its wings so slowly that their movements can be followed by the eye ; and under the conditions just mentioned they can be seen in all positions, either vertically or horizontally, the bird apparently remaining stationary for several minutes together, although in reality flying just as fast as the steamer is going. When a Gull is viewed from the side, it is easily seen that the stroke of the wing, from the shoulder, is vertically up and down, or very nearly so ; and when observed directly over head, it is still more easily seen that, at each downward stroke, the primary feathers from the carpal or wrist-joint are moved backward, slightly closing the wing, but without any perceptible bending of the elbow-joint. I cannot, of course, say exactly at what time the backward stroke begins, only that it is principally delivered during the downward stroke, which agrees fairly enough with Prof. Marey's experiments, which show that the back-stroke commences soon after the down-stroke has begun, and finishes soon after the up-stroke has begun. The mechanical principles here employed are obvious. The support of the bird in the air is partly obtained by the concave under surface of the wing offering more resistance to the air during the downward stroke than the convex upper surface does during the upward stroke. The rise of the bird during the down-stroke, and its fall during the up-stroke are quite perceptible when viewed horizontally ; its flight, however, would be nothing more than a series of tremendous jerks upward and downward, which, even with immense exertion, would hardly raise it in the air, if it were not that by far

the greater part of its support is derived from its forward progress, as I have already explained when describing the sailing flight of the Albatros (*Phil. Mag.*, Aug. 1869). This forward progress is obtained by the backward stroke, or rowing motion, of the primaries from the carpal joint, which, combined with the downward movement, makes the feathers press on the air in a downward and backward direction. While, therefore, the movement of the main part of the wing from the shoulder is vertical, the tips, by having also a horizontal movement, do not describe an undulating line in the air, but a cycloidal curve, thus confirming the experiments of Prof. Marey.

No twisting of the wing on its axis is perceptible by the eye; but such a movement probably takes place; for the anatomical investigations of Dr. Pettigrew show that "during flexion the anterior margin is slightly directed downwards, and in extension decidedly directed upwards" (*l. c.* p. 241). It is, I think, the erroneous idea that flexion must occur during the up-stroke that has led Dr. Pettigrew astray in his theory of the flight of birds. A moment's consideration will show that it is during the up-stroke, which must tend to depress the bird, that the largest surface of wing is required to take advantage of the progressive movement obtained by the down-stroke, and so, by acting like a kite, prevent the bird from falling. If the flexion of the wing during the down-stroke be allowed, and this I have distinctly seen, the discrepancy in the description of the movement of the wings by Dr. Pettigrew and Prof. Marey, which I have already pointed out, disappears.

Dr. Pettigrew's experiments, Nos. 14 and 18, show that when the primary feathers, or the tips of the wings, are much shortened, flight is stopped, or much impeded in birds, but not in insects, the reason of this difference being that the latter cannot bend their wings, but obtain forward progression in quite a different manner to birds. Experiments Nos. 12 and 13 show that if the primaries are left entire, or nearly so, flight is but little impaired, although the greater part of the other feathers may be cut off. This is because the primaries are the portion of the wing by which progression is obtained, and it is progression that principally supports the bird; for without progression, either

upward or onward, no bird can remain in the air. The same principle also explains why flight is nearly perfect when the tip of one wing only is cut off; for enough still remains for progression, and although the impulse must be greater on one side than on the other, still the bird can easily correct this when progression is once obtained. Experiment No. 19, however, which shows that when the carpal joints are rendered immoveable flight is entirely prevented, although the wings in all other respects are perfect, goes far, I think, to prove the correctness of the views here advanced; and if another experiment should be made in which the elbow-joints were fixed, while the carpal joints were quite free, and the action of the muscles unimpeded, and the bird then found to fly with ease, all doubts on the subject would, I think, disappear.

Wellington, New Zealand,
October 16th, 1871.

P.S.—Since the above was written I have had the pleasure of reading Prof. Marey's valuable and clearly written lectures "On the Phenomena of Flight in the Animal Kingdom," translated for the Smithsonian Institution ('Smithsonian Reports,' 1869). It will be seen that the results of M. Marey's experiments are fully borne out by my observations on the Sea-gull in its natural state, the only point on which I differ from him being the way in which progression is obtained; and his opinion in this case, is not derived from experiment, but from theoretical considerations only, while mine is from direct observation. If, in M. Marey's theory, the backward stroke of the primary feathers be substituted for the uplifting of the posterior margin of the wing by the resistance of the air, I believe that the true principles in the flight of birds will be thoroughly understood, the only thing remaining to be done being to obtain experimentally formulæ for the resistance of the air to the front and under surfaces of birds when the wings are fully expanded, as in sailing flight.